DRAFT SUPPLEMENT TO REPORT ON THE FATAL SHOT

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What our experiment has shown is that one can not argue from general principles that the fatal shot must have come from the front. That is, it is not true that, as I and many other critics had thought, and as Thompson set forth in detail, if the bulk of a target is seen to move in one direction, it follows from the laws of physics that the shot must have come from the opposite direction. Our melon tests provided a counter-example to that theory. In fact, even without the experiment, the calculations in Appendix 1 show that once the possibility of high-momentum, low-mass fragments is considered, the general argument (as set forth by Thompson) is not valid.

There should be no disagreement about the previous paragraph. But all it says is that one particular argument ~~[illegible]~~ that the shot came from the front is not valid. That does not, of course, prove that the shot did not come from the front. To see what can be inferred about the assassination, we must consider several questions:

1) Does Thompson's argument, in a more restricted form, apply to a human skull, even though it does not apply to all targets?

2) Are any of the details of the motion in the Zapruder film, or its magnitude, inconsistent with a shot from the rear and the "jet recoil" mechanism?

3) What assessment can be made of the evidence on the direction of the shot, other than the motion ? (That is, medical reports, witnesses, etc.)

When we first decided to do an experiment to see if a recoil was not only consistent with momentum conservation (as we knew it was) but also physically possible, Alvarez suggested as a target a melon strengthened with tape. It should be noted that we did not go around shooting at various targets until we found one that worked. We felt that a taped melon and a head would have in common the following essential features: a shell strong enough to resist shattering (i.e., we expected that if a melon were taped it would develop reasonably small entry and exit holes), and contents that were soft but not liquid.

When melons were unavailable, and to try out other targets, we shot at targets without these features, and we feel that our observations suggest that our intuition about the necessary properties was correct. An untaped melon or a coconut typically shattered, leaving only small fragments scattered in many directions. Water-filled plastic bottles tended to go forward, but not directly forward; the large amount of water sprayed out made it difficult to tell if there was a jet recoil effect. The gelatin in a rubber ball was, I suggest, too cohesive to jet out. We filmed one untaped pineapple being shot, and although it shattered one large piece was driven backwards and another went perpendicular to the bullet path: this is not what one would have expected from a naive application of Newton's laws, but on the other hand we did not see a recoil of most of the target. (That is why I call these results "inconclusive" - not because they did not prove what we "wanted" them to prove.)

There is only one way to see if a jet effect can be produced in a human skull, and that is to shoot one. At this time, I can only offer my opinion: I would be quite surprised if a human skull could not produce the same kind of jet effect as that we have observed with melons.

Assuming for the moment that a jet effect is possible in a head, we ask if there are reasons for questioning the extrapolation of our results to a head. That is, are the obvious differences between the two targets important enough to outweigh the similarities? We first consider the question in the abstract, making no reference to what is seen in the Zapruder film. I conclude that one can not a priori expect the magnitude and details of the jet-recoil effect to be similar in the two cases, and in fact they might be expected to be quite different. Here we must consider the possible mechanisms responsible for the effect, and how they might depend on the details of the situation.

The jetting is undoubtedly caused at least in part by the effect of the bullet as it passes through the target at a high speed. Although I have no detailed knowledge of shock wave phenomena, it seems probable that the effect is directly related not only to such properties of the medium as its viscosity and compressibility, but also to the speed of sound in that medium. The speed of sound generally increases with density; it is about 1100 feet per second in air, 4500 fps in water or gelatin, and 1400 fps in a very light solid, cork. The muzzle velocity of a Carcano bullet is about 2000 fps; the bullets we used in our tests may have been up to 3000 fps. It seems that after being slowed as it enters the target, a bullet would have a velocity well under that of sound in either a melon or a head (assuming that the speed of sound in these cases is closer to that in water than cork); however, the speed of the bullet and the speed of sound are not very widely separated, so I would not predict that a melon and a head would necessarily behave the same way.

The above-described mechanism might produce a jet effect even with a very rigid shell on the target. (Consider, for example, a very stong metal container with a very fast bullet required to penetrate the shell.) With a non-rigid container, another mechanism might come into play: deformation of the shell and consequent displacement of the contents. If we push on a deformable container whose contents are not very compressible (e.g., water), and which has a hole on the opposite side, some of the contents will be forced out, even if we push slowly. Certainly the shell of a melon can be deformed in that way, and such deformation may contribute significantly to the jet effect we observed. I suspect that a skull deforms without shattering under the impact of a bullet to a surprisingly large degree. Nonetheless, it is quite possible that this deformation mechanism produced a large part of the jetting observed in the melons but would not do so in a skull.

Finally, [illegible] note that an observable recoil requires a forward jet strong enough to overcome whatever forward motion is transferred to the target by the bullet impact. The calculation in Appendix 1 of the original draft report shows that not much of the incident kinetic energy energy needs to be used up to produce the forward jet. However, a transiting bullet may lose a great deal of its energy, in other ways. As the bullet enters a target, it is slowed down, transferring some of the energy it loses to the target, driving it forward. If there is no jet recoil, of course this forward motion is what is observed. A jet-recoil effect may appear to counteract this forward momentum almost instantaneously, but the net motion certainly depends on the amount of energy and momentum transfered when the bullet strikes the shell as it enters. (Energy transfer as the bullet or its fragments exit has a similar effect.)

Perhaps a simple "Gedanken experiment" will make this clear. Consider two targets, A and B, with identical contents, A having a much thicker shell. The bullet that strikes A should have a much greater speed than the one striking B, so that after penetrating the shell both bullets have the same speed. Let the targets both have thin shells on the side opposite the bullet impact; then the recoil momentum produced by jetting will be the same in both cases. However, in case A the impact will produce a large transfer of momentum forward, and the net effect will be forward motion; in case B, the backward recoil will dominate.

We did not attempt to measure the entrance or exit velocity of the bullet going through the melon, but it seems likely to me that the energy loss was small compared to that expected in a skull. (The comparison is confused by the fact that we used bullets with higher speed than Carcano bullets; our bullets were soft-nosed, not fully jacketed. It is not clear to me what effect this would have on the energy lost, but there certainly could be a difference. Soft-nosed and full-jacketed bullets characteristically fragment quite differently. The question of whether the fragmentation observed in the assassination is consistent with a full-jacketed bullet is discussed below.)

In summary: the similarities between a melon and a head are enough to suggest that a recoil is possible in the latter case. That is, I would be quite surprised if a test with a skull showed that it could not support some forward jetting. (This is obviously a matter of my own judgment. This decision may be wrong, but it is not particularly "unscientific." It is unfortunate that the mathematics and technicalities of language involved in, for example, my own field of high-energy physics obscure from the outsider the extent to which progress in science is based on conflicting opinions and interpretations, not unambiguous facts.) However, the differences between the two targets and bullets used are significant enough so that I would not deduce from the melon tests that a bullet of the type allegedly used could produce a significant recoil under the actual conditions of the assassination.

I feel that further experiments would not add much unless the conditions of the assassination were carefully duplicated. The proper bullets are available, but an appropriate target may not be easy to find. John Nichols has noted that sheep or cow skulls have strong butressing ridges, unlike human skulls, so they would not be good simulated targets. Howard Roffman has pointed out that an old human skull (as was used in the tests for the Warren Commission) would probably be more brittle than a living one. Perhaps a series of tests using a variety of targets could provide a persuasive argument about what would happen to a skull, but it would be necessary to try various combinations of material to simulate the skull, brain, and skin, and study their properties in some detail. It is not clear, for example, what should be used to simulate brain matter. Gelatin was used in the Edgewood Aresnal tests (5H76,87), but the testimony does not say in what respects it is considered a good simulation. It may have similar stopping power but different "jetting" ability because of its cohesiveness. I do not have access to the facilities for an experiment which could not only simulate a head as a target but also check the fragmentation and wounds for comparison with those observed in the assassination.

Incidentally, such facilities may have been available to both the Warren Commission and CBS. Edgewood Arsenal had done studies of the "wounding characteristics of various bullets" (5H76), and had used cadavers for some tests. (5H74)

From my own brief scan of the literature on high-speed photography, I gather that bullet impact studies form one of the major practical applications, and that such work has been done at Edgewood. (My impression is that such studies are primarily concerned with the fine details of impact with a metal target, and are applied to the development of protective armor, etc.) One of the most prominent workers in high-spped photography is Harold Edgerton, founder of E.G. & G., for whom Charles Wyckoff was working when he did his study for CBS.

Although we cannot extrapolate a priori from our tests to the Zapruder film, we can consider if what the film shows is consistent with the recoil effect hypothesis. As we have noted, the general similarity is striking. In both cases there appears to be a small entry wound and a large exit wound. Significant expulsion of material from the front of the target is observed in both cases. (It is unclear how much material is thrown backwards in the Zapruder film; this question is complicated by the relative motion of the car with respoect to the road, and also by the wind. The melon tests do show that there can be some backward jetting through the entry hole and still a net forward motion.)

Although the recoil velocity is comparable in the two cases, the recoil momentum is quite different. With the melons, we observed a recoil momentum of something less than 18 ft-lb/sec (= 6 ft/sec x 3 lb). From the Zapruder film, I estimated the recoil momentum to be between roughly 32 and 96 ft-lb/sec. (See Appendix 1 of the draft report.) The bullet momentum was about 40 ft-lb/sec for a Carcano bullet, and perhaps 30% higher in our tests. Thus, the recoil momentum was well under the incident momentum in the melon tests, but about as large or perhaps twice as large as the incident momentum in the assassination. Of course, we would not have expected the melon to recoil back with as large a momentum, since its weight was rather small, but on the other hand it is quite possible that the jet recoil effect does not permit a recoil with momentum as large as or larger than that of the incident bullet. We have not tested that hypothesis.

It should be noted that these must be some limit on the recoil momentum.

if there were not (that is, if *all* of the incident kinetic energy could be transfered to forward-going and recoiling fragments of the target), a bullet like that allegedly used in the Carcano, with 37000 ft-lb/sec^2 of energy, could, upon impact with a 40-lb. target, drive 20 pounds forward at about 43 feet per second, and the same amount of matter backwards at essentially the same speed. This conserves energy and momentum, but is clearly "unphysical." In the absence of a detailed understanding of the mechanism and of tests to establish what this limit is, my "educated guess" as a physicist would be that the maximum possible recoil momentum is of the same order of magnitude as the incident momentum. (Here "order of magnitude" denotes roughly within a factor of ten.) Since the melon tests and the assassination have recoil momenta on the opposite sides of this guess for the limit (although both are reasonably close to it), I would hesitate to *predict* that the large backward momentum seen in the assassination is necessarily consistent with a recoil effect. (On the other hand, it should be noted that a backward momentum larger than the incident momentum also could not be due to a similar bullet fired from the front; that is, a bullet from the front producing a backward momentum of 96 ft-lb/sec (our higher estimate) would would have to be quite a bit heavier or faster than the alleged assassination bullets. I have no idea what velocities or masses are associated with the various "special" bullets that have been speculated about, or what kind of damage they might cause.)

I do not see anything in the details of the motion in the Zapruder film that strikes me as *necessarily* inconsistent with the recoil hypothesis. I have referred to the motion as "backwards", as opposed to "forward," but I am well aware that there is significant motion to the left. (This is not very clear in the Zapruder film, but can be seen in the Nix film and others, I am told.) If a recoil mechanism can produce motion directly toward the gun rather than away from it, it can certainly produce motion left or right; this depends on the deflection of the bullet in the target and the location of the exit hole (or the previously existing weak spot) through which the jetting occurs. The evidence is persuasive that the President's major head wounds were on the right side, which would lead us to expect a leftward component in the recoil. As I have indicated, I am not convinced that the forward motion between frames 312 and 313 is present, especially as strongly as Thompson's measurements show, but I see no way of arguing that such forward motion would be inconsistent with a shot from the rear. It is quite plausible to me that a single shot from the rear would produce first some forward motion and then a backwards recoil.

In order to assess the probability that the President was shot only from the rear, one must consider evidence other than the motion. Even if the motion is completely consistent with a rear shot, as appears likely to me, it is hardly conclusive by itself. To take a hypothetical case, if the pathologists had said that the wounds alone were inconclusive as to the number and direction of the shots, and if a rifle had been found on the grassy knoll, I would most certainly not conclude from the motion and our tests that the shot probably came from the rear. The other evidence will be taken into account, explicitly or implicitly, by anyone who assesses the melon tests, and I want to set forth here my own evaluation of it.

There is some evidence positively suggestive of a shot from the front. The eye- and ear-witness testimony of suspicious activity on the grassy knoll is well known, and extensive. Thompson cites some such evidence which directly supports the double-hit theory. S.M. Holland, for example, had a strong opinion that the third and fourth shots both hit Kennedy. (I do not find this persuasive; since he claims these two shots were close together and were different in character, he may have heard both the report from the rifle and the sound of impact from a single shot.) Thompson also notes that DPD Officer James Chaney said he saw Kennedy struck in the face. (Page 103.) There is quite a bit of such evidence, but I tend to consider it relatively unreliable, and suggest that we should focus our attention on the "hard" physical evidence, as far as it is available.

There is considerable evidence that impact debris from the fatal shot was driven to the left and/or the rear. Thompson cites the testimony of Altgens and Brehm (pages 99-100). Officer Hargis, who was riding at the left rear, thought at first he might have been hit by the last shot. (Thompson, page 100). The Nix film shows some impact debris on the car trunk. As I have indicated, with melons some jetting back through the entry hole is possible; given the small size of the rear wound in Kennedy's head, substantial backward jetting seems unlikely. The most reasonable explanation for the observed distribution of blood and brain matter seems to me to be that it was driven forward and up rather slowly; then the motion of the car and the wind would have a significant effect. (Note that 10 miles per hour = 14.7 feet per second; a fine spray of material could easily be up in the air for a good fraction of a second.) (Thompson suggests that Hargis was hit "with such force" that he thought he may have been hit (page 100), but the cited sources do not suggest that he was hit hard by the impact debris.)

The location and nature of fragments of bone which were found are closer to "hard" physical evidence. At least one such fragment was found by Officer Weitzman near the left curb of Elm Street. (I haven't checked out the chain of possession, but I think this is one of the pieces given to the doctors during the autopsy. Perhaps this was the fragment that Brehm said he saw going left and to the rear. Under the circumstances, I place no weight on Brehm's detailed description of the trajectory. However, the exact location at which this fragment was found may be known: Garrison publicized some photos of what he called a Federal agent picking up a bullet from the grass on the south side of Elm Street very shortly after the shooting. In rebuttal, if my memory serves me correctly, it was claimed that the photos showed a local police officer picking up bone fragments. The exact location can be determined from these photos, which show the TSBD in the background; at the time, I convinced myself that it appeared to be more or less in a straight line with the TSBD window and the car's position at frame 313. Because of the curvature of the road, such a line intersects the south curb of Elm near the sewer inlet. (Dr. Boswell told Thompson that the fragment showing the beveling of an exit wound which was presented to the doctors during the autopsy could be located at "the very crown of the President's head at the midline" (page 109). Gary Schoener is, I think, referring to this fragment when he points out that the one found in the grass is from the juncture of the parietal and occipital bones, which would be expected to be driven far to the left rear by a shot from the front. I think that bone from this location - rather close to the rear entry wound - could also be driven stongly forward and to the left curb of Elm by a shot from the rear. Perhaps a close correlation of frames 312 through 314 with the medical reports would show whether one of the clearly visible large fragments streaking through the air might have come from that part of the skull.)

As far as I know, neither the Commission, the autopsy doctors, or the Review Panel considered the bone fragment found by William Harper on the south side of Elm on November 23. The pathologist who described this bone fragment for the FBI in Dallas said that it looked like it came from the occipital (rear) region of the skull - which, for any substantial fragment, would be inconsistent with any of the official versions of the President's wounds. Not knowing the reason for this identification, or how unambiguous it could be expected to be, I don't think it can be given much weight. However, an examination of the photos of this fragment (which are withheld in the Archives, CD 1269) is necessary to confirm or refute this claim.

I now turn to the case for a single shot from the rear, and the problems with that case which relate directly to the question of a frontal shot. I am quite aware that all the medical evidence must be weighed skeptically, given the unquestionably bad job done by the original autopsy doctors, the Commission, and the 1968 Review Panel. If the burning of Humes' first draft went unquestioned, how can any of the medical evidence be trusted? I am not willing to rule out even massive fraud, tampering with the photos, and the like. For this analysis I will assume that the facts reported by the various sources are reasonably accurate, although I am not willing to accept the interpretation given them by the various doctors.

All sources agree on the general nature of the President's massive wounds, and deduce that the incoming bullet fragmented severely. Several people have claimed that the full-jacketed, rather slow bullet allegedly used does not fragment in such a fashion. Even Dr. Olivier, who did the tests on the skull wounds for the Commission, did not expect such fragmentation, but rather a small entry and exit. (5H87) On this point, I can only accept the opinion of a source (who has asked that his name not be used) who has told me that he did not at first believe that the bullet could leave a large number of small fragments in the head, with major portions deflected out at high speed, but is now sure that it can happen. I consider this information reliable, and will try to get more details.

I am reasonably convinced that there was a small entry wound in the back of the President's head. The 1967 Autopsy Doctors' Review noted that

> Due to the fractures of the underlying bone and the elevation of the scalp by manual lifting (done to permit the wound to be photographed) the photographs show the wound to be slightly higher than its actually measured site. (Page 3)

The Panel Review noted that both the photos and the X-rays place the wound about 100 mm (4 inches) above the occipital protruberance. This is clearly inconsistent with the original report of "slightly above," but is in fact more consistent with the angle of a shot from the Depository. (See Z312 vs. CE 388; Thompson, page 111.) (Despite this, the Autopsy Doctors' Review said that the photos verified the accuracy of CE 388!)

In arguing against the Commission's case, Sylvia Meagher noted that just one of the 16 medical personnel who saw Kennedy at Parkland and the four federal agents present at the autopsy noted the alleged entry wound. That one, Roy Kellerman, placed it in the hairline to the right of the right ear. Assuming that Kellerman meant "left," I do not find this all particularly surprising, if the revised location for the wound, according to the Panel Review, is correct.

As evidence that this was an entry wound, the Warren Report cites only the beveling observed in the autopsy. The Autopsy Doctors' Review admits that it is not particularly obvious as an entry wound:

> The scalp wound shown in the photographs appears to be a laceration and tunnel, with the actual penetration of the skin obscured by the top of the tunnel. [Whatever that means - P.H.] From the photographs this is not recognizable as a penetrating wound because of the slanting direction of entry. (Page 3)

They confirmed that they had noted a characteristic beveling of the underlying bone. The Review Panel sort of confirmed this beveling. No conclusions were drawn from the photos of the front and the inside of the skull from the front, since they were lacking in clarity and detail. The X-rays showed that the entry hole was as much as 20 mm. in diameter on the inside surface of the skull, as opposed to "approximately 8 mm. in diameter" on the outer surface. (Page 11.) This is exactly the beveling effect said to be characteristic of an entry wound. However, the original autopsy report described the wound as being 6 by 15 mm. on the outside, and (besides the inconsistency) beveling from 15 to 20 mm. would hardly seem as convincing as that from 8 to 20 mm. In any case, what was observed was certainly not as unambiguous as Dr. Finck's chart (CE 400), so I would not accept the evidence on beveling as conclusive at this point.

The Panel Review cited (for the first time, I think) additional evidence concerning the area near this wound: a large (6.5 mm. in diameter) fragment of metal in the outer table of the skull, localized elevation of the soft tissues next to the hole, and small fragments of bone in the area. (Page 11) There is other evidence suggestive of a rear-to-front passage. Thompson points out that the exit wound which shows beveling is at the top of the head and in itself indeterminative of the direction of impact. I am not in a position to evaluate this kind of evidence. However, there is one piece of evidence that, in the opinion of Dr. Cyril Wecht, is (if accurately reported) conclusive evidence that the rear wound was one of entry: there was "coagulation necrosis of the tissues at the wound margins" (16H988, Thompson, page 280).

When I wrote the draft report on the melon tests, I was generally familiar with all of the above arguments. My opinion was then, and remains now, that the evidence cited for a shot from the front is not persuasive, and (making generous but reasonable choices between the contradictory evidence in several areas) I would accept the basic argument for a single shot from the rear, although that was not exactly ironclad. I have since come across two arguments that, in my opinion, significantly weaken any confidence one can have in the recoil theory. The first argument involves a problem that I was aware of when I wrote Appendix 1, and which was pointed out to me by several people; it involves the details of the damage to the brain tissue.

Despite the numerous reports suggesting that large quantities of brain matter were displaced by the bullet, and the evidence of some being driven out of the head in the few frames after impact, it is not clear that enough was ejected to carry off the required momentum. Even after the expulsion of brain matter by the shot, and the probable subsequent loss before and during treatment at Parkland, the brain weighed 1500 grams. (16H987) This was after formalin fixation, and I have no idea if this procedure adds significantly to the apparent weight of the brain; I do not know if the measurement should be assumed to be accurate to within, say plus or minus 50 grams; and I do not know how much the weight of a normal brain varies from the average of 1400 grams. (My source for this figure is the Encyclopedia Britannica, which gives an average weight of 1409 grams for the male, 1263 for the female. Among unusually intelligent people, 1830 grams is cited as unusually large and 1077 grams as unusually small. Thus I would guess that 1400 ± 200 grams is a reasonable range among normal people. At autopsy, the recorded brain weight was 1450 grams for Oswald (24H9) and 1350 for Tippit (SS 563, p. 2).) The supplemental autopsy report (16H987) notes "considerable" loss of cortical substance, as well as marked disruption of the right cerebral hemisphere, but the measured weight is consistent with the loss of only a very small fraction of the brain. Conservation of momentum could be satisfied even if essentially no brain matter were lost, if the bone fragments were driven forward fast enough, but it is not easy for me to see that this could happen without the "jet effect" in the brain matter similar to what we observed in the melons. As I noted, perhaps a third to a half of the melon was jetted out. The autopsy photos should show whether or not a substantial amount of brain matter was actually lost; perhaps the existing description allow one to make a reasonably precise estimate, but I can not do so.

(Incidentally, on page A1.3 of the original draft report, I estimated the amount of matter driven forward as something less than 1 pound; this is clearly much too large an upper limit, by a factor of four or so. I still have not estimated the weight of the skull fragments. Despite this overestimate of the upper limit, I want to emphasize again that the numerical calculation in that appendix was just an order-of-magnitude calculation and cannot be interpreted as proving what did or did not happen.)

The other argument which, I think, requires that more evidence should be studied before a decision is reached on the probability of the recoil theory concerns possible positive evidence of a shot from the front. Dick Bernabei has set forth for me a detailed and intriguing argument that the pattern of metal fragments in the President's head is not consistent with a single shot from the rear. This argument was new to me, and when I checked it against the autopsy and Panel reports I found that the available medical evidence does not effectively rebut Bernabei's case. In brief, his argument is as follows:

The Panel Review describes two separate sets of metal fragments in the brain - some relatively large ones, distributed more or less randomly, and fine dust-like fragments concentrated in the right front portion of the head. When a bullet fragments on impact, it can produce both large and small fragments, but the small ones do not travel as far as the larger ones. Thus, the presence of very small fragments at the right front - and the apparent absence of such fragments elsewhere in the head - proves that there was an impact in the right front area.

Bernabei presents the claim that small fragments do not travel as far as large ones as an empirical fact, well known to experts; he has provided me with an illustration showing the fragmentation of a hollow-point (bursting) bullet in gelatin. Such behavior is what I would expect from a naive physical argument: the stopping force on a projectile passing through a substance like gelatin might be expected to be proportional to the exposed area of the projectile. For a spherical projectile, this area is proportional to the square of the radius. The distance traveled by a particle of a given velocity is proportional to its momentum divided by the stopping force. The momentum is proportional to the mass; hence to the volume of the sphere, and hence to the cube of the radius. Thus the distance is proportional to the radius: for fragments of a given speed, one would expect the larger ones to travel further. (Of course, the exact relationahip is not as simple as this argument suggests.)

What Bernabei's argument can prove is only that the small fragments must have been *created* near where they came to rest. If the observed distribution is consistent with a single shot from the rear, the fine fragments must have been deposited by a large fragment from the original bullet, as it passed forward in the head.

I do not find it implausible that a fragment could deposit small, slow particles along its path in such a fashion. That is, in fact, the hypothesis presented by both the original autopsy doctors and the Review Panel. However, these sources differ significantly in their description of the evidence on this point.

The autopsy report noted that

> Roentgenograms of the skull reveal multiple minute metallic fragments along a line corresponding with a line joining the above described small occipital wound and the right supra-orbital ridge. (WR 541)

(In the extant draft of this report, it can be seen that "small occipital wound" was originally "occipital puncture wound" (17H38).) Humes testified that

> These [the X-rays] had disclosed to us multiple minute fragments of radio opaque material traversing a line from the wound in the occiput to just above the right eye, with a rather sizable fragment visible by X-ray just above the right eye. These tiny fragments that were seen dispersed through the substance of the brain in between were, in fact, just that extremely minute, less than 1 mm. in size for the most part. (2H353)

From the Panel Review:

> Distributed through the right cerebral hemisphere are numerous small, irregular metallic fragments, most of which are less than 1 mm. in maximum dimension.... [They] fall into two groups. One group consists of relatively large fragments, more or less randomly distributed. The second group consists of finely divided fragments, distributed in a postero-anterior direction in a region 45 mm. long and 8 mm. wide.

The Panel Review makes clear what the autopsy doctors did not: that the fine fragments form a distinct cluster at the very front of the head, extending back only about 1.8 inches. (The Panel Review notes that this formation "appears to end anteriorly immediately below the badly fragmented frontal and parietal bones just anterior to the region of the coronal suture." (Page 11.)) Bernabei points out that the Panel Review reported no such small fragments anywhere else in the head. I do not take that as conclusive proof that there were no such other fragments, but further examination of the X-rays is certainly required. I do not know anything about the behavior of fragments from a fully-jacketed bullet, and whether it is reasonable to assume that such a fragment could deposit dust-like bits of metal only near the end of its path. Such behavior would probably require that the fragment was a piece of lead and copper which turned around, exposing the softer lead to frictional forces, as it moved. This is conceivable, but hardly probable. The hypothesis that bullets can behave in that was should be tested experimentally before any conclusions are drawn.

A second important question, in addition to the degree of concentration of the fragments in the front, is the direction of the observed cluster. The autopsy report and Humes' testimony give the picture of a quite clear connection between the fragments at the front and the rear wound. The report of the FBI agents observing the autopsy similarly gives the impression of a clear line of fragments: "/̲ the X-rays _7 disclosed a path of a missile which appeared to enter the back of the skull and the path of the disintegrated fragments could be observed along the right side of the skull" (CD 7, p. 283). The Panel Review does confirm that the cluster was narrow: 8 mm. is only a little more than the bullet diameter. However, the Panel says only that

> As seen on lateral film #2 this formation overlies the position of the coronal suture; its long axis if extended posteriorly passes through the above mentioned hole. (Page 11, emphasis added.)

Since the cluster is less than two inches long, one is entitled to question whether the inference drawn from the extension of its axis is in fact as unambiguous as the Panel suggests. I don't know what kind of precision can be expected in the reading of such photos and X-rays, but since the entry wound itself shows up four inches away from "its actually measured site," I would need to be convinced that the fragment cluster really points right back through it. By referring specifically to lateral film #2, the Panel suggests that we ask what the cluster looks like on the other X-rays, if any show it. Perhaps only this one shows it clearly, but on the other hand maybe the others show that in another plane its axis does not point back to the entry hole.

If these small particles were deposited by a large fragment, what happened to it? Humes' testimony connects the cluster with "a rather sizable fragment visible by X-ray just above the right eye" (2H353). In fact, two relatively large fragments (7 x 2 and 3 x 1 mm.) were removed from this general area. Referring to CE 388, Humes testified that

> A second portion of the missile or multiple second portions were deflected, and traversed a distance as enumerated by this interrupted line, with the

> major portion of that fragment coming to lodge in the position indicated. Perhaps some of these minor fragments were dislodged from the major one as it traversed this course. (2H353-4)

The Panel Report makes no specific mention of these two substantial fragments. That doesn't necessarily mean that there are no such fragments visible on the X-rays, since the Panel did not enumerate all the major fragments. However, the Panel definitely did not associate the cluster with any large fragments, as Humes had done. On the contrary, the Panel Report says that the large fragment which deposited this cluster exited from the head. In addition to noting that the cluster ended just below "badly fragmented" bones, as quoted above, the Report said

> The projectile fragmented on entering the skull, one major section leaving a trail of fine metallic debris as it passed forward and laterally to explosively fracture the right frontal and parietal bones as it emerged from the head. (Page 12)

I really don't have the knowledge to evaluate all these apparent contradictions properly. Perhaps ambiguity at this level is not unusual in the analysis of X-rays and autopsy photos. An expert might be able to suggest further questions for consideration. For example, is the damage to the brain as shown in the photos and described in the clinical records consistent with a fragment in the hypothesized rear-to-front path - that is, is a detectable channel to be expected, and was one seen? Is the damage at the cluster consistent with entrance, exit, or both?

If one takes into account only the circumstances of the autopsy and the relative qualifications of the autopsy doctors and the review panelists, one would be inclined to give greater weight to the Panel Report than to the doctors' reports. However, both the circumstances of the Panel Report and its manner of presenting its conclusions make it just as unacceptable as the earlier reports. Practically every sentence reflects a choice of words that might be acceptable in a legal brief but hardly suggests an investigation conducted and reported in an unbiased way. (The entire report is analyzed in Post-Mortem, Part III, by Harold Weisberg.) A few of the strange things in the Report relate directly to the question of the direction and number of the head shots. It is interesting to note that the "inventory of material examined" by the Panel does not include CE 843, the two fragments described above which were removed from the head. (But the inventory does include CE 840, three fragments of lead found on a rug in the President's car, about which the Panel report says nothing at all!) A detailed examination of these fragments might have been revealing. As various critics have noted, there is no meaningful spectrographic analysis of the bullets available. Either it was not done or it was suppressed. We have only meaningless statements such as various "lead fragments were similar in composition" (5H67). Although this kind of analysis might not be able to prove that various fragments did come from the same bullet, it certainly might provide conclusive proof that they came from different bullets.

One of the major conclusions of the Panel was phrased very carefully. They presented reasonably persuasive evidence that no bullet passed through the left side of the head. (Although there were skull fractures and some damage to the brain on that side, as well as damage behind the left eyelid, there were no metallic fragments or holes through the skull on that side.) The conclusion was that no projectile "passed through the head in any direction other than from back to front" (Page 15). It can be argued that the Panel was just taking note of the fact that some of the skull was missing, and it was not possible to rule out an entry wound on those portions of bone. Although the Report does not address the possibility of a second, non-penetrating, bullet directly, I expect that this possibility was considered when the wording "passed through", rather than "entered", was used (twice). In other words, the Report does not consider the hypothesis that the cluster resulted from a frontal impact, and writes a conclusion that does not deny that possibility. It would be interesting to find out if this hypothesis was considered in any of the Panel's deliberations.

It is not at all obvious that the cluster of fine fragments is consistent with a shot from the front. Bernabei admits that the cluster could not constitute, in weight, the major portion of a bullet. Perhaps the fragment exit noted at the top of the head is consistent with an impact in the right front. I have no idea if various "special" bullets could account for what was observed. As noted above, such a bullet would have to be of very high speed or mass to provide the large backwards impulse; it would have to be very soft to produce primarily small fragments on impact. In connection with such unusual hypothetical special bullets, Gary Schoener has pointed out that quite a few witnesses smelled gunpowder at street level, which is suggestive of a bullet containing an explosive charge. Also, there might be some connection between a special bullet and the "gray brown rectangular structure measuring approximately 12 x 20 mm" which can be seen in the photos of the brain. The Review Panel said only that "its identity cannot be established by The Panel" (Page 8).

Until the photos and X-rays are examined by qualified experts who are specifically looking for alternatives to the Commission's hypotheses, I can not consider the medical evidence conclusive. That is, I would not say there is persuasive evidence for a shot from the front, but the Commission's case is hardly persuasive either. As discussed above, we cannot deduce from the melon tests that a skull could sustain a recoil as large as that observed in the Zapruder film when struck by the bullet allegedly used; that is, one can think of several things that might make this recoil impossible, but they have not been tested one way or the other. Thus, it would be premature to conclude that the jet effect hypothesis can adequately reconcile the observed facts with a single rear shot. (My original draft report certainly gave that erroneous impression.) The jet effect hypothesis does, I think, remove an argument against the Commission's version of the assassination that I had considered simple, direct, and persuasive. (Despite my biases in the other direction, I have a hunch that a single rear shot with a jet effect will turn out to be the correct explanation, but I would certainly not want to present this opinion as a direct logical consequence of the melon tests.)

END

degrees per frame. On top of this smooth panning, add an oscillation of 2 degrees per frame per frame. That is, the velocity due to the oscillation is (assume it starts after 4 frames) 0,0,0,0,-2,0,2,0,-2,0,2.... (These numbers do not represent a sinusoidal oscillation, but don't worry about that.) Adding the two velocities, one finds a net velocity of 2,2,2,2,0,4,2,0,2,4.... A net velocity of 2 means that the background is blurred but the car is not; 0 means that the car is blurred but the background is not; 4 means both are blurred.

The actual situation is more complicated than this simplified example suggests (e.g., motion in all three directions may be present). It is clear, however, that to measure the forces exerted by Zapruder on the camera, one may measure the change in the blurring of highlights in various frames.

Dr. Alvarez thought of this analysis in November 1966, and made some measurements on the frames in Life and Volume 18. He brought this matter to the attention of people he knew at CBS, and suggested that they have the work confirmed by an independent scientist. (CBS selected Dr. Charles Wyckoff of E.G. & G.) Even at that early stage of the analysis, Alvarez noted an oscillation starting around frame 180. CBS relied heavily on this as evidence for an early shot. Without getting into a detailed critique of CBS's work, I would accept this as probative evidence of such a shot. I feel that there is a great deal of evidence which suggests an early shot, such as the reaction of Mrs. Kennedy and other witnesses in the Zapruder film. Some critics suggest that the early shot hit Kennedy. As evidence for an early shot, the detailed measurement of the acceleration by CBS and Alvarez does not allow one to make a stronger statement than does the simple observation that there is suddenly a lot of blurring in the film. This observation was made in one of the first books on the assassination. Harold Weisberg noted (Whitewash, pages 47-48) that the film becomes suddenly fuzzy at frame 190, and attributed this to motion of the camera resulting from Zapruder's emotional reaction to what he had seen. Weisberg also pointed out that Zapruder testified (7H751) that he heard the first shot and saw the President lean over and grab himself, which suggests that the shot came well before the President disappeared from Zapruder's view about frame 210.

(Incidentally, this book had been made available to CBS while they were working on their report. Thompson notes that an analysis along the same lines as Alvarez' was considered and rejected as probably inconclusive by Life in November 1966. However, Alvarez' work was completely independent. Until it was televised I did not even know that he was doing such a study.)

The main argument about this "jiggle theory" is not about the probative evidence for an early shot, but about the claim that it can prove that there were three and only three shots. If the film is a sensitive enough indirect record of shock waves, it could provide an accurate count of the number of shots. Before coming to such a conclusion, as CBS did, I think it is necessary to consider all of the following questions:

(A) Is the oscillation seen on the film characteristic of the reaction to a shot?

(B) To what extent can other effects produce similar oscillations?

(1) What is the "noise level" - that is, if an undisturbed person holds a camera "steady", how much acceleration is measured by the technique of examining blurs? How often, if at all, does the oscillatory pattern associated with a shot occur if there is no disturbance?

(2) Do imperfections in the camera mechanism produce blurring? (Thompson said (page 293) that the experts at Life felt that some blurs were so caused.)

(3) Does a conscious reaction (e.g., a small change in the panning) or a small disturbance (crowd noises, etc.) produce a reaction like that of a shot?

(C) Under what conditions will a shot not be recorded by this method?

(1) Will a shot with a weak sound at Zapruder's location produce a recognizable signal? (This is important if one wants to rule out, say, a shot

from the TSBD that went wild.) Would a shot from a weapon other than an ordinary rifle, or one with a "silencer", muffle a shot enough to produce no measureable reaction?

(2) Perhaps the most important question: what is the "resolution time" of this method of detecting shots? That is, what is the minimum separation time for two closely-spaced shots that will still be recorded as distinct? There are two different aspects to this problem:

(a) Does the neuromuscular reaction which triggers the oscillations have a non-negligible "dead time"? Perhaps only the first of a series of closely spaced shots will cause jiggling, with subsequent ones not triggering a new reaction.

(b) If a new oscillation is started while a previous one is dying away, can it be detected with measurements of whatever precision is available, or will it just look like an extension of the previous oscillations, with no discernible starting point?

There is no doubt that each of these two imperfections in the method does make some contribution to the resolution time. (I have not tried to calculate what these times might be.) This is not just a minor quibble, since the observed oscillations which have been attributed to shots extend over a large time - on the order of a second. If a pair of shots a second apart would be missed by this method, there is no way of claiming that there were only a certain number of shots.

Alvarez and CBS did consider point (A) adequately, I think. Tests were made by firing shots near cameramen who were told to hold steady as they were filming. Although this test did not reproduce the conditions of the assassination, it was close enough. Also, Alvarez noted the pattern of blurring after frame 313, where there certainly was a shot, and saw a general similarity to the two other patterns which he attributed to shots. This justifies the conclusion that the film strongly suggests an early shot.

As far as I know, CBS did not do any separate tests on point (B). They did examine the very early frames of the film, from which a "noise level" can be estimated. However, I feel that the arguments which CBS used to distinguish the three alleged shots from other oscillations do not stand up.

On the broadcast CBS report, Wyckoff almost said that the characteristic blurring pattern occurs only three times:

> RATHER:... Now, how many times does that occur in this ten-second stretch of 8 mm film?
> WYCKOFF: Now, that - that occurs several times, as we've represented up here on the - on this film on the wall. It occurs at frame 190, it occurs again at frame 227. Frame 227 was the next evidence that I had of Mr. Zapruder moving his camera. And it occurs at frame 318.... (White, p. 228)

White's text (pp. 75-6) makes clear that there were other candidates for shots:

> Mr. Wyckoff, meanwhile, reported that he had found similar streaking [to that on the test film] at three points of the Zapruder film [frames 190, 227, and 318] and other streaking, far less marked and easily distinguishable from the principal streaking, in two other places on the film.

I have never seen Wyckoff's actual measurements, and am unwilling to simply accept his judgment, as reported by White, as to which streaking was "principal." The problem CBS set for itself was not to identify shots, knowing that there were three, but to try to determine how many there were. White provides some further analysis of the two "non-shots"-

> The two minor disturbances also included one point of interest. They were represented on a chart drawn by Mr. Wyckoff as he proceeded with his tests. It may be seen that the disturbances at two particular frames are significantly less noticeable, in terms of the length and duration of the streaking. [I don't have this chart.] One frame provides nothing further than its own mute evidence. But the other frame shows a slight, almost imperceptible slowing down of the President's car, followed almost immediately by resumption of its normal parade speed. Professor Alvarez hypothesizes that both the deceleration and the twitch of Mr. Zapruder's hand were brought about by a siren, which caused the driver instinctively to lift his foot from the accelerator ... and at the same time slightly startled Mr. Zapruder. A tenuous hypothesis, to be sure, but not unreasonable, The earlier slight disturbance in the first frame was in all possibility (!) caused by still another unexpected sound, which cannot be identified. [A gunshot, maybe?] In that fashion, all the evidence recorded on the Zapruder film is accountable. (Page 76-77)

Alvarez measured the car's speed, and did find that it slows down somewhat near frame 290, where one of the weak oscillations starts. I independently confirmed that the car slowed down, but I could not pin the deceleration down to a single frame or so, and found no evidence of a subsequent rapid return to

DRAFT SUPPLEMENT TO REPORT ON THE FATAL SHOT

Paul L. Hoch
October 23, 1979

What our experiment has shown is that one can not argue from general principles that the fatal shot must have come from the front. That is, it is not true that, as I and many other critics had thought, and as Thompson set forth in detail, if the bulk of a target is seen to move in one direction, it follows from the laws of physics that the shot must have come from the opposite direction. Our melon tests provided a counter-example to that theory. In fact, even without the experiment, the calculations in Appendix 1 show that once the possibility of high-momentum, low-mass fragments is considered, the general argument (as set forth by Thompson) is not valid.

There should be no disagreement about the previous paragraph. But all it says is that one particular argument that the shot came from the front is not valid. That does not, of course, prove that the shot did not come from the front. To see what can be inferred about the assassination, we must consider several questions:

1) Does Thompson's argument, in a more restricted form, apply to a human skull, even though it does not apply to all targets?

2) Are any of the details of the motion in the Zapruder film, or its magnitude, inconsistent with a shot from the rear and the "jet recoil" mechanism?

3) What assessment can be made of the evidence on the direction of the shot, other than the motion ? (That is, medical reports, witnesses, etc.)

When we first decided to do an experiment to see if a recoil was not only consistent with momentum conservation (as we knew it was) but also physically possible, Alvarez suggested as a target a melon strengthened with tape. It should be noted that we did not go around shooting at various targets until we found one that worked. We felt that a taped melon and a head would have in common the following essential features: a shell strong enough to resist shattering (i.e., we expected that if a melon were taped it would develop reasonably small entry and exit holes), and contents that were soft but not liquid.

When melons were unavailable, and to try out other targets, we shot at targets without these features, and we feel that our observations suggest that our intuition about the necessary properties was correct. An untaped melon or a coconut typically shattered, leaving only small fragments scattered in many directions. Water-filled plastic bottles tended to go forward, but not directly forward; the large amount of water sprayed out made it difficult to tell if there was a jet recoil effect. The gelatin in a rubber ball was, I suggest, too cohesive to jet out. We filmed one untaped pineapple being shot, and although it shattered one large piece was driven backwards and another went perpendicular to the bullet path: this is not what one would have expected from a naive application of Newton's laws, but on the other hand we did not see a recoil of most of the target. (That is why I call these results "inconclusive" - not because they did not prove what we "wanted" them to prove.)

There is only one way to see if a jet effect can be produced in a human skull, and that is to shoot one. At this time, I can only offer my opinion: I would be quite surprised if a human skull could not produce the same kind of jet effect as that we have observed with melons.

Assuming for the moment that a jet effect is possible in a head, we ask if there are reasons for questioning the extrapolation of our results to a head. That is, are the obvious differences between the two targets important enough to outweigh the similarities? We first consider the question in the abstract, making no reference to what is seen in the Zapruder film. I conclude that one can not a priori expect the magnitude and details of the jet-recoil effect to be similar in the two cases, and in fact they might be expected to be quite different. Here we must consider the possible mechanisms responsible for the effect, and how they might depend on the details of the situation.

The jetting is undoubtedly caused at least in part by the effect of the bullet as it passes through the target at a high speed. Although I have no detailed knowledge of shock wave phenomena, it seems probable that the effect is directly related not only to such properties of the medium as its viscosity and compressibility, but also to the speed of sound in that medium. The speed of sound generally increases with density; it is about 1100 feet per second in air, 4500 fps in water or gelatin, and 1400 fps in a very light solid, cork. The muzzle velocity of a Carcano bullet is about 2000 fps; the bullets we used in our tests may have been up to 3000 fps. It seems that after being slowed as it enters the target, a bullet would have a velocity well under that of sound in either a melon or a head (assuming that the speed of sound in these cases is closer to that in water than cork); however, the speed of the bullet and the speed of sound are not very widely separated, so I would not predict that a melon and a head would necessarily behave the same way.

The above-described mechanism might produce a jet effect even with a very rigid shell on the target. (Consider, for example, a very stong metal container with a very fast bullet required to penetrate the shell.) With a non-rigid container, another mechanism might come into play: deformation of the shell and consequent displacement of the contents. If we push on a deformable container whose contents are not very compressible (e.g., water), and which has a hole on the opposite side, some of the contents will be forced out, even if we push slowly. Certainly the shell of a melon can be deformed in that way, and such deformation may contribute significantly to the jet effect we observed. I suspect that a skull deforms without shattering under the impact of a bullet to a surprisingly large degree. Nonetheless, it is quite possible that this deformation mechanism produced a large part of the jetting observed in the melons but would not do so in a skull.

Finally, note that an observable recoil requires a forward jet strong enough to overcome whatever forward motion is transferred to the target by the bullet impact. The calculation in Appendix 1 of the original draft report shows that not much of the incident kinetic energy energy needs to be used up to produce the forward jet. However, a transiting bullet may lose a great deal of its energy, in other ways. As the bullet enters a target, it is slowed down, transferring some of the energy it loses to the target, driving it forward. If there is no jet recoil, of course this forward motion is what is observed. A jet-recoil effect may appear to counteract this forward momentum almost instantaneously, but the net motion certainly depends on the amount of energy and momentum transfered when the bullet strikes the shell as it enters. (Energy transfer as the bullet or its fragments exit has a similar effect.)

Perhaps a simple "Gedanken experiment" will make this clear. Consider two targets, A and B, with identical contents, A having a much thicker shell. The bullet that strikes A should have a much greater speed than the one striking B, so that after penetrating the shell both bullets have the same speed. Let the targets both have thin shells on the side opposite the bullet impact; then the recoil momentum produced by jetting will be the same in both cases. However, in case A the impact will produce a large transfer of momentum forward, and the net effect will be forward motion; in case B, the backward recoil will dominate.

We did not attempt to measure the entrance or exit velocity of the bullet going through the melon, but it seems likely to me that the energy loss was small compared to that expected in a skull. (The comparison is confused by the fact that we used bullets with higher speed than Carcano bullets; our bullets were soft-nosed, not fully jacketed. It is not clear to me what effect this would have on the energy lost, but there certainly could be a difference. Soft-nosed and full-jacketed bullets characteristically fragment quite differently. The question of whether the fragmentation observed in the assassination is consistent with a full-jacketed bullet is discussed below.)

In summary: the similarities between a melon and a head are enough to suggest that a recoil is possible in the latter case. That is, I would be quite surprised if a test with a skull showed that it could not support some forward jetting. (This is obviously a matter of my own judgment. This decision may be wrong, but it is not particularly "unscientific." It is unfortunate that the mathematics and technicalities of language involved in, for example, my own field of high-energy physics obscure from the outsider the extent to which progress in science is based on conflicting opinions and interpretations, not unambiguous facts.) However, the differences between the two targets and bullets used are significant enough so that I would not deduce from the melon tests that a bullet of the type allegedly used could produce a significant recoil under the actual conditions of the assassination.

I feel that further experiments would not add much unless the conditions of the assassination were carefully duplicated. The proper bullets are available, but an appropriate target may not be easy to find. John Nichols has noted that sheep or cow skulls have strong butressing ridges, unlike human skulls, so they would not be good simulated targets. Howard Roffman has pointed out that an old human skull (as was used in the tests for the Warren Commission) would probably be more brittle than a living one. Perhaps a series of tests using a variety of targets could provide a persuasive argument about what would happen to a skull, but it would be necessary to try various combinations of material to simulate the skull, brain, and skin, and study their properties in some detail. It is not clear, for example, what should be used to simulate brain matter. Gelatin was used in the Edgewood Aresnal tests (5H76,87), but the testimony does not say in what respects it is considered a good simulation. It may have similar stopping power but different "jetting" ability because of its cohesiveness. I do not have access to the facilities for an experiment which could not only simulate a head as a target but also check the fragmentation and wounds for comparison with those observed in the assassination.

Incidentally, such facilities may have been available to both the Warren Commission and CBS. Edgewood Arsenal had done studies of the "wounding characteristics of various bullets" (5H76), and had used cadavers for some tests. (5H74)

From my own brief scan of the literature on high-speed photography, I gather that bullet impact studies form one of the major practical applications, and that such work has been done at Edgewood. (My impression is that such studies are primarily concerned with the fine details of impact with a metal target, and are applied to the development of protective armor, etc.) One of the most prominent workers in high-speed photography is Harold Edgerton, founder of E.G. & G., for whom Charles Wyckoff was working when he did his study for CBS.

Although we cannot extrapolate a priori from our tests to the Zapruder film, we can consider if what the film shows is consistent with the recoil effect hypothesis. As we have noted, the general similarity is striking. In both cases there appears to be a small entry wound and a large exit wound. Significant expulsion of material from the front of the target is observed in both cases. (It is unclear how much material is thrown backwards in the Zapruder film; this question is complicated by the relative motion of the car with respect to the road, and also by the wind. The melon tests do show that there can be some backward jetting through the entry hole and still a net forward motion.)

Although the recoil velocity is comparable in the two cases, the recoil momentum is quite different. With the melons, we observed a recoil momentum of something less than 18 ft-lb/sec (= 6 ft/sec x 3 lb). From the Zapruder film, I estimated the recoil momentum to be between roughly 32 and 96 ft-lb/sec. (See Appendix 1 of the draft report.) The bullet momentum was about 40 ft-lb/sec for a Carcano bullet, and perhaps 30% higher in our tests. Thus, the recoil momentum was well under the incident momentum in the melon tests, but about as large or perhaps twice as large as the incident momentum in the assassination. Of course, we would not have expected the melon to recoil back with as large a momentum, since its weight was rather small, but on the other hand it is quite possible that the jet recoil effect does not permit a recoil with momentum as large as or larger than that of the incident bullet. We have not tested that hypothesis.

It should be noted that these must be some limit on the recoil momentum.

If there were not (that is, if all of the incident kinetic energy could be transfered to forward-going and recoiling fragments of the target), a bullet like that allegedly used in the Carcano, with 37000 ft-lb/sec^2 of energy, could, upon impact with a 40-lb. target, drive 20 pounds forward at about 43 feet per second, and the same amount of matter backwards at essentially the same speed. This conserves energy and momentum, but is clearly "unphysical." In the absence of a detailed understanding of the mechanism and of tests to establish what this limit is, my "educated guess" as a physicist would be that the maximum possible recoil momentum is of the same order of magnitude as the incident momentum. (Here "order of magnitude" denotes roughly within a factor of ten.) Since the melon tests and the assassination have recoil momenta on the opposite sides of this guess for the limit (although both are reasonably close to it), I would hesitate to predict that the large backward ~~recoil~~ momentum seen in the assassination is necessarily consistent with a recoil effect. (On the other hand, it should be noted that a backward momentum larger than the incident momentum also could not be due to a similar bullet fired from the front; that is, a bullet from the front producing a backward momentum of 96 ft-lb/sec (our higher estimate) would would have to be quite a bit heavier or faster than the alleged assassination bullets. I have no idea what velocities or masses are associated with the various "special" bullets that have been speculated about, or what kind of damage they might cause.)

I do not see anything in the details of the motion in the Zapruder film that strikes me as necessarily inconsistent with the recoil hypothesis. I have referred to the motion as "backwards", as opposed to "forward," but I am well aware that there is significant motion to the left. (This is not very clear in the Zapruder film, but can be seen in the Nix film and others, I am told.) If a recoil mechanism can produce motion directly toward the gun rather than away from it, it can certainly produce motion left or right; this depends on the deflection of the bullet in the target and the location of the exit hole (or the

previously existing weak spot) through which the jetting occurs. The evidence is persuasive that the President's major head wounds were on the right side, which would lead us to expect a leftward component in the recoil. As I have indicated, I am not convinced that the forward motion between frames 312 and 313 is present, especially as strongly as Thompson's measurements show, but I see no way of arguing that such forward motion would be inconsistent with a shot from the rear. It is quite plausible to me that a single shot from the rear would produce first some forward motion and then a backwards recoil.

In order to assess the probability that the President was shot only from the rear, one must consider evidence other than the motion. Even if the motion is completely consistent with a rear shot, as appears likely to me, it is hardly conclusive by itself. To take a hypothetical case, if the pathologists had said that the wounds alone were inconclusive as to the number and direction of the shots, and if a rifle had been found on the grassy knoll, I would most certainly not conclude from the motion and our tests that the shot probably came from the rear. The other evidence will be taken into account, explicitly or implicitly, by anyone who assesses the melon tests, and I want to set forth here my own evaluation of it.

There is some evidence positively suggestive of a shot from the front. The eye- and ear-witness testimony of suspicious activity on the grassy knoll is well known, and extensive. Thompson cites some such evidence which directly supports the double-hit theory. S.M. Holland, for example, had a strong opinion that the third and fourth shots both hit Kennedy. (I do not find this persuasive; since he claims these two shots were close together and were different in character, he may have heard both the report from the rifle and the sound of impact from a single shot.) Thompson also notes that DPD Officer James Chaney said he saw Kennedy struck in the face. (Page 103.) There is quite a bit of such evidence, but I tend to consider it relatively unreliable, and suggest that we should focus our attention on the "hard" physical evidence, as far as it is available.

There is considerable evidence that impact debris from the fatal shot was driven to the left and/or the rear. Thompson cites the testimony of Altgens and Brehm (pages 99-100). Officer Hargis, who was riding at the left rear, thought at first he might have been hit by the last shot. (Thompson, page 100). The Nix film shows some impact debris on the car trunk. As I have indicated, with melons some jetting back through the entry hole is possible; given the small size of the rear wound in Kennedy's head, substantial backward jetting seems unlikely. The most reasonable explanation for the observed distribution of blood and brain matter seems to me to be that it was driven forward and up rather slowly; then the motion of the car and the wind would have a significant effect. (Note that 10 miles per hour = 14.7 feet per second; a fine spray of material could easily be up in the air for a good fraction of a second.) (Thompson suggests that Hargis was hit "with such force" that he thought he may have been hit (page 100), but the cited sources do not suggest that he was hit hard by the impact debris.)

The location and nature of fragments of bone which were found are closer to "hard" physical evidence. At least one such fragment was found by Officer Weitzman near the left curb of Elm Street. (I haven't checked out the chain of possession, but I think this is one of the pieces given to the doctors during the autopsy. Perhaps this was the fragment that Brehm said he saw going left and to the rear. Under the circumstances, I place no weight on Brehm's detailed description of the trajectory. However, the exact location at which this fragment was found may be known: Garrison publicized some photos of what he called a Federal agent picking up a bullet from the grass on the south side of Elm Street very shortly after the shooting. In rebuttal, if my memory serves me correctly, it was claimed that the photos showed a local police officer picking up bone fragments. The exact location can be determined from these photos, which show the TSBD in the background; at the time, I convinced myself that it appeared to be more or less in a straight line with the TSBD window and the car's position at

frame 313. Because of the curvature of the road, such a line intersects the south curb of Elm near the sewer inlet. (Dr. Boswell told Thompson that the fragment showing the beveling of an exit wound which was presented to the doctors during the autopsy could be located at "the very crown of the President's head at the midline" (page 109). Gary Schoener is, I think, referring to this fragment when he points out that the one found in the grass is from the juncture of the parietal and occipital bones, which would be expected to be driven far to the left rear by a shot from the front. I think that bone from this location - rather, close to the rear entry wound - could also be driven stongly forward and to the left curb of Elm by a shot from the rear. Perhaps a close correlation of frames 312 through 314 with the medical reports would show whether one of the clearly visible large fragments streaking through the air might have come from that part of the skull.)

As far as I know, neither the Commission, the autopsy doctors, or the Review Panel considered the bone fragment found by William Harper on the south side of Elm on November 23. The pathologist who described this bone fragment for the FBI in Dallas said that it looked like it came from the occipital (rear) region of the skull - which, for any substantial fragment, would be inconsistent with any of the official versions of the President's wounds. Not knowing the reason for this identification, or how unambiguous it could be expected to be, I don't think it can be given much weight. However, an examination of the photos of this fragment (which are withheld in the Archives, CD 1269) is necessary to confirm or refute this claim.

I now turn to the case for a single shot from the rear, and the problems with that case which relate directly to the question of a frontal shot. I am quite aware that all the medical evidence must be weighed skeptically, given the unquestionably bad job done by the original autopsy doctors, the Commission, and the 1968 Review Panel. If the burning of Humes' first draft went unquestioned, how can any of the medical evidence be trusted? I am not willing to rule out even massive fraud, tampering with the photos, and the like. For this analysis I will assume that the facts reported by the various sources are reasonably accurate, although I am not willing to accept the interpretation given them by the various doctors.

All sources agree on the general nature of the President's massive wounds, and deduce that the incoming bullet fragmented severely. Several people have claimed that the full-jacketed, rather slow bullet allegedly used does not fragment in such a fashion. Even Dr. Olivier, who did the tests on the skull wounds for the Commission, did not expect such fragmentation, but rather a small entry and exit. (5H87) On this point, I can only accept the opinion of a source (who has asked that his name not be used) who has told me that he did not at first believe that the bullet could leave a large number of small fragments in the head, with major portions deflected out at high speed, but is now sure that it can happen. I consider this information reliable, and will try to get more details.

I am reasonably convinced that there was a small entry wound in the back of the President's head. The 1967 Autopsy Doctors' Review noted that

> Due to the fractures of the underlying bone and the elevation of the scalp by manual lifting (done to permit the wound to be photographed) the photographs show the wound to be slightly higher than its actually measured site. (page 3)

The Panel Review noted that both the photos and the X-rays place the wound about 100 mm (4 inches) above the occipital protruberance. This is clearly inconsistent with the original report of "slightly above," but is in fact more consistent with the angle of a shot from the Depository. (See Z312 vs. CE 388; Thompson, page 111.) (Despite this, the Autopsy Doctors' Review said that the photos verified the accuracy of CE 388!)

In arguing against the Commission's case, Sylvia Meagher noted that just one of the 16 medical personnel who saw Kennedy at Parkland and the four federal agents present at the autopsy noted the alleged entry wound. That one, Roy Kellerman, placed it in the hairline to the right of the right ear. Assuming that Kellerman

meant "left," I do not find this all particularly surprising, if the revised location for the wound, according to the Panel Review, is correct.

As evidence that this was an entry wound, the Warren Report cites only the beveling observed in the autopsy. The Autopsy Doctors' Review admits that it is not particularly obvious as an entry wound:

> The scalp wound shown in the photographs appears to be a laceration and tunnel, with the actual penetration of the skin obscured by the top of the tunnel. [Whatever that means - P.H.] From the photographs this is not recognizable as a penetrating wound because of the slanting direction of entry. (page 3)

They confirmed that they had noted a characteristic beveling of the underlying bone. The Review Panel sort of confirmed this beveling. No conclusions were drawn from the photos of the front and the inside of the skull from the front, since they were lacking in clarity and detail. The X-rays showed that the entry hole was as much as 20 mm. in diameter on the inside surface of the skull, as opposed to "approximately 8 mm. in diameter" on the outer surface. (Page 11.) This is exactly the beveling effect said to be characteristic of an entry wound. However, the original autopsy report described the wound as being 6 by 15 mm. on the outside, and (besides the inconsistency) beveling from 15 to 20 mm. would hardly seem as convincing as that from 8 to 20 mm. In any case, what was observed was certainly not as unambiguous as Dr. Finck's chart (CE 400), so I would not accept the evidence on beveling as conclusive at this point.

The Panel Review cited (for the first time, I think) additional evidence concerning the area near this wound: a large (6.5 mm. in diameter) fragment of metal in the outer table of the skull, localized elevation of the soft tissues next to the hole, and small fragments of bone in the area. (Page 11) There is other evidence suggestive of a rear-to-front passage. Thompson points out that the exit wound which shows beveling is at the top of the head and in itself indeterminative of the direction of impact. I am not in a position to evaluate this kind of evidence. However, there is one piece of evidence that, in the opinion of Dr. Cyril Wecht, is (if accurately reported) conclusive evidence that the rear wound was one of entry: there was "coagulation necrosis of the tissues at the wound margins" (16H988, Thompson, page 210).

When I wrote the draft report on the melon tests, I was generally familiar with all of the above arguments. My opinion was then, and remains now, that the evidence cited for a shot from the front is not persuasive, and (making generous but reasonable choices between the contradictory evidence in several areas) I would accept the basic argument for a single shot from the rear, although that was not exactly ironclad. I have since come across two arguments that, in my opinion, significantly weaken any confidence one can have in the recoil theory. The first argument involves a problem that I was aware of when I wrote Appendix 1, and which was pointed out to me by several people; it involves the details of the damage to the brain tissue.

Despite the numerous reports suggesting that large quantities of brain matter were displaced by the bullet, and the evidence of some being driven out of the head in the few frames after impact, it is not clear that enough was ejected to carry off the required momentum. Even after the expulsion of brain matter by the shot, and the probable subsequent loss before and during treatment at Parkland, the brain weighed 1500 grams. (16H987) This was after formalin fixation, and I have no idea if this procedure adds significantly to the apparent weight of the brain; I do not know if the measurement should be assumed to be accurate to within, say plus or minus 50 grams; and I do not know how much the weight of a normal brain varies from the average of 1400 grams. (My source for this figure is the Encyclopedia Britannica, which gives an average weight of 1409 grams for the male, 1263 for the female. Among unusually intelligent people, 1830 grams is cited as unusually large and 1077 grams as unusually small. Thus I would guess that 1400 ± 200 grams is a reasonable range among normal people. At autopsy, the recorded brain weight was 1450 grams for Oswald (24H9) and 1350 for Tippit (SS 563, p. 2).) The supplemental autopsy report (16H987) notes considerable" loss of cortical substance, as well as marked disruption of the right cerebral hemisphere, but the measured weight is consistent with the loss of only a very small fraction of the brain. Conservation of momentum could be satisfied even if essentially no brain matter were lost, if the bone fragments were driven forward fast enough, but it is not easy for me to see that this could happen without the "jet effect" in the brain matter similar to what we observed in the melons. As I noted, perhaps a third to a half of the melon was jetted out. The autopsy photos should show whether or not a substantial amount of brain matter was actually lost; perhaps the existing description allow one to make a reasonably precise estimate, but I can not do so.

(Incidentally, on page A1.3 of the original draft report, I estimated the amount of matter driven forward as something less than 1 pound; this is clearly much too large an upper limit, by a factor of four or so. I still have not estimated the weight of the skull fragments. Despite this overestimate of the upper limit, I want to emphasize again that the numerical calculation in that appendix was just an order-of-magnitude calculation and cannot be interpreted as proving what did or did not happen.)

The other argument which, I think, requires that more evidence should be studied before a decision is reached on the probability of the recoil theory concerns possible positive evidence of a shot from the front. Dick Bernabei has set forth for me a detailed and intriguing argument that the pattern of metal fragments in the President's head is not consistent with a single shot from the rear. This argument was new to me, and when I checked it against the autopsy and panel reports I found that the available medical evidence does not effectively rebut Bernabei's case. In brief, his argument is as follows:

The Panel Review describes two separate sets of metal fragments in the brain - some relatively large ones, distributed more or less randomly, and fine dust-like fragments concentrated in the right front portion of the head. When a bullet fragments on impact, it can produce both large and small fragments, but the small ones do not travel as far as the larger ones. Thus, the presence of very small fragments at the right front - and the apparent absence of such fragments elsewhere in the head - proves that there was an impact in the right front area.

Bernabei presents the claim that small fragments do not travel as far as large ones as an empirical fact, well known to experts; he has provided me with an illustration showing the fragmentation of a hollow-point (bursting) bullet in gelatin. Such behavior is what I would expect from a naive physical argument: the stopping force on a projectile passing through a substance like gelatin might be expected to be proportional to the exposed area of the projectile. For a spherical projectile, this area is proportional to the square of the radius. The distance traveled by a particle of a given velocity is proportional to its momentum divided by the stopping force. The momentum is proportional to the mass, hence to the volume of the sphere, and hence to the cube of the radius. Thus the distance is proportional to the radius: for fragments of a given speed, one would expect the larger ones to travel further. (Of course, the exact relationahip is not as simple as this argument suggests.)

What Bernabei's argument can prove is only that the small fragments must have been created near where they came to rest. If the observed distribution is consistent with a single shot from the rear, the fine fragments must have been deposited by a large fragment from the original bullet, as it passed forward in the head.

I do not find it implausible that a fragment could deposit small, slow particles along its path in such a fashion. That is, in fact, the hypothesis presented by both the original autopsy doctors and the Review Panel. However, these sources differ significantly in their description of the evidence on this point.

The autopsy report noted that

> Roentgenograms of the skull reveal multiple minute metallic fragments along a line corresponding with a line joining the above described small occipital wound and the right supra-orbital ridge. (WR 541)

(In the extant draft of this report, it can be seen that "small occipital wound" was originally "occipital puncture wound" (17H38).) Humes testified that

> These [the X-rays] had disclosed to us multiple minute fragments of radio opaque material traversing a line from the wound in the occiput to just above the right eye, with a rather sizable fragment visible by X-ray just above the right eye. These tiny fragments that were seen dispersed through the substance of the brain in between were, in fact, just that extremely minute, less than 1 mm. in size for the most part. (2H353)

From the Panel Review:

> Distributed through the right cerebral hemisphere are numerous small, irregular metallic fragments, most of which are less than 1 mm. in maximum dimension.... [They] fall into two groups. One group consists of relatively large fragments, more or less randomly distributed. The second group consists of finely divided fragments, distributed in a postero-anterior direction in a region 45 mm. long and 8 mm. wide.

The Panel Review makes clear what the autopsy doctors did not: that the fine fragments form a distinct cluster at the very front of the head, extending back only about 1.8 inches. (The Panel Review notes that this formation "appears to end anteriorly immediately below the badly fragmented frontal and parietal bones just anterior to the region of the coronal suture." (Page 11.)) Bernabei points out that the Panel Review reported no such small fragments anywhere else in the head. I do not take that as conclusive proof that there were no such other fragments, but further examination of the X-rays is certainly required. I do not know anything about the behavior of fragments from a fully-jacketed bullet, and whether it is reasonable to assume that such a fragment could deposit dust-like bits of metal only near the end of its path. Such behavior would probably require that the fragment was a piece of lead and copper which turned around, exposing the softer lead to frictional forces, as it moved. This is conceivable, but hardly probable. The hypothesis that bullets can behave in that was should be tested experimentally before any conclusions are drawn.

A second important question, in addition to the degree of concentration of the fragments in the front, is the direction of the observed cluster. The autopsy report and Humes' testimony give the picture of a quite clear connection between the fragments at the front and the rear wound. The report of the FBI agents observing the autopsy similarly gives the impression of a clear line of fragments: "/ the X-rays _/ disclosed a path of a missile which appeared to enter the back of the skull and the path of the disintegrated fragments could be observed along the right side of the skull" (CD 7, p. 283). The Panel Review does confirm that the cluster was narrow: 8 mm. is only a little more than the bullet diameter. However, the Panel says only that

> As seen on lateral film #2 this formation overlies the position of the coronal suture; its long axis _if_ _extended_ _posteriorly_ passes through the above mentioned hole. (Page 11, emphasis added.)

Since the cluster is less than two inches long, one is entitled to question whether the inference drawn from the extension of its axis is in fact as unambiguous as the Panel suggests. I don't know what kind of precision can be expected in the reading of such photos and X-rays, but since the entry wound itself shows up four inches away from "its actually measured site," I would need to be convinced that the fragment cluster really points right back through it. By referring specifically to lateral film #2, the Panel suggests that we ask what the cluster looks like on the other X-rays, if any show it. Perhaps only this one shows it clearly, but on the other hand maybe the others show that in another plane its axis does not point back to the entry hole.

If these small particles were deposited by a large fragment, what happened to it? Humes' testimony connects the cluster with "a rather sizable fragment visible by X-ray just above the right eye" (2H353). In fact, two relatively large fragments (7 x 2 and 3 x 1 mm.) were removed from this general area. Referring to CE 388, Humes testified that

> A second portion of the missile or multiple second portions were deflected, and traversed a distance as enumerated by this interrupted line, with the major portion of that fragment coming to lodge in the position indicated. Perhaps some of these minor fragments were dislodged from the major one as it traversed this course. (2H353-4)

The Panel Report makes _no_ specific mention of these two substantial fragments. That doesn't necessarily mean that there are no such fragments visible on the X-rays, since the Panel did not enumerate all the major fragments. However, the Panel definitely did not associate the cluster with any large fragments, as Humes had done. On the contrary, the Panel Report says that the large fragment which deposited this cluster exited from the head. In addition to noting that the cluster ended just below "badly fragmented" bones, as quoted above, the Report said

> The projectile fragmented on entering the skull, one major section leaving a trail of fine metallic debris as it passed forward and laterally to explosively fracture the right frontal and parietal bones as it emerged from the head. (Page 12)

I really don't have the knowledge to evaluate all these apparent contradictions properly. Perhaps ambiguity at this level is not unusual in the analysis of X-rays and autopsy photos. An expert might be able to suggest further questions for consideration. For example, is the damage to the brain as shown in the photos and described in the clinical records consistent with a fragment in the hypothesized rear-to-front path - that is, is a detectable channel to be expected, and was one seen? Is the damage at the cluster consistent with entrance, exit, or both?

If one takes into account only the circumstances of the autopsy and the relative qualifications of the autopsy doctors and the review panelists, one would be inclined to give greater weight to the Panel Report than to the doctors' reports. However, both the circumstances of the Panel Report and its manner of presenting its conclusions make it just as unacceptable as the earlier reports. Practically every sentence reflects a choice of words that might be acceptable in a legal brief but hardly suggests an investigation conducted and reported in an unbiased way. (The entire report is analyzed in _Post-Mortem_, Part III, by Harold Weisberg.) A few of the strange things in the Report relate directly to the question of the direction and number of the head shots. It is interesting to note that the "inventory of material examined" by the Panel does not include CE 843, the two fragments described above which were removed from the head. (But the inventory does include CE 840, three fragments of lead found on a rug in the President's car, about which the Panel report says nothing at all!) A detailed examination of these fragments might have been revealing. As various critics have noted, there is no meaningful spectrographic analysis of the bullets available. Either it was not done or it was suppressed. We have only meaningless statements such as various "lead fragments were similar in composition" (5H67). Although this kind of analysis might not be able to prove that various fragments did come from the same bullet, it certainly might provide conclusive proof that they came from different bullets.

One of the major conclusions of the Panel was phrased very carefully. They presented reasonably persuasive evidence that no bullet passed through the left side of the head. (Although there were skull fractures and some damage to the brain on that side, as well as damage behind the left eyelid, there were no metallic fragments or holes through the skull on that side.) The conclusion was that no projectile "passed through the head in any direction other than from back to front" (Page 15). It can be argued that the Panel was just taking note of the fact that some of the skull was missing, and it was not possible to rule out an entry wound on those portions of bone. Although the Report does not address the possibility of a second, non-penetrating, bullet directly, I expect that this possibility was considered when the wording "passed through", rather than "entered", was used (twice). In other words, the Report does not consider the hypothesis that the cluster resulted from a frontal impact, and writes a conclusion that does not deny that possibility. It would be interesting to find out if this hypothesis was considered in any of the Panel's deliberations.

It is not at all obvious that the cluster of fine fragments is consistent with a shot from the front. Bernabei admits that the cluster could not constitute,

in weight, the major portion of a bullet. Perhaps the fragment exit noted at the top of the head is consistent with an impact in the right front. I have no idea if various "special" bullets could account for what was observed. As noted above, such a bullet would have to be of very high speed or mass to provide the large backwards impulse; it would have to be very soft to produce primarily small fragments on impact. In connection with such unusual hypothetical special bullets, Gary Schoener has pointed out that quite a few witnesses smelled gunpowder at street level, which is suggestive of a bullet containing an explosive charge. Also, there might be some connection between a special bullet and the "gray brown rectangular structure measuring approximately 12 x 20 mm" which can be seen in the photos of the brain. The Review Panel said only that "its identity cannot be established by The Panel" (Page 8).

Until the photos and X-rays are examined by qualified experts who are specifically looking for alternatives to the Commission's hypotheses, I can not consider the medical evidence conclusive. That is, I would not say there is persuasive evidence for a shot from the front, but the Commission's case is hardly persuasive either. As discussed above, we cannot deduce from the melon tests that a skull could sustain a recoil as large as that observed in the Zapruder film when struck by the bullet allegedly used; that is, one can think of several things that might make this recoil impossible, but they have not been tested one way or the other. Thus, it would be premature to conclude that the jet effect hypothesis can adequately reconcile the observed facts with a single rear shot. (My original draft report certainly gave that erroneous impression.) The jet effect hypothesis does, I think, remove an argument against the Commission's version of the assassination that I had considered simple, direct, and persuasive. (Despite my biases in the other direction, I have a hunch that a single rear shot with a jet effect will turn out to be the correct explanation, but I would certainly not want to present this opinion as a direct logical consequence of the melon tests.)

LMS

Appendix- THE ANALYSIS OF BLURRING PATTERNS IN THE ZAPRUDER FILM

This so-called "jiggle theory" was mentioned only incidentally in the draft report on the head motion problem: as a background note describing Alvarez' original interest in the assassination, and in a list of evidence suggesting an early shot. In this appendix I want to summarize this theory and outline my own opinion on what has and has not been established.

CBS and Alvarez argued that Zapruder reacted to the shots with involuntary oscillations of the camera, at a characteristic frequency; that these oscillations produced changes in the pattern of blurring in successive frames, which could be measured; and that analysis of these blurring patterns shows that three and only three shots were fired.

Since CBS completely misrepresented the basic argument presented to it by Alvarez, I will detail it here. Blurring of part or all of a single frame is not evidence of a shot. The size of the image of a small bright point (i.e., the degree of blurring) is proportional to the angular velocity of the point relative to the camera. If Zapruder were panning his camera exactly with the car, the highlights on the car would not be blurred, but those in the stationary background would be. Suppose a shot starts left-right oscillations of the camera. As the camera swings to the left, the panning motion is in effect cancelled out; if the camera is stationary when a frame is exposed, the background will not be blurred but the car will be.

This is really just a problem in classical (Gallilean) relativity. To study the torques (forces) that are producing the oscillation, one wants to look at the angular acceleration (which is proportional to the torque). This acceleration (rate of change of velocity) is proportional to the change in the length of blurs in successive frames. The essentially constant angular velocity of the car cancels out when this difference is taken.

A simple numerical example may make this clear. Suppose that the car and the camera are both going to the right with a constant angular velocity of 2

degrees per frame. On top of this smooth panning, add an oscillation of 2 degrees per frame ~~xx~~ per frame. That is, the velocity due to the oscillation is (assume it starts after 4 frames) 0,0,0,0,-2,0,2,0,-2,0,2.... (These numbers do not represent a sinusoidal oscillation, but don't worry about that.) Adding the two velocities, one finds a net velocity of 2,2,2,2,0,4,2,0,2,4.... A net velocity of 2 means that the background is blurred but the car is not; 0 means that the car is blurred but the background is not; 4 means both are blurred.

The actual situation is more complicated than this simplified example suggests (e.g., motion in all three directions may be present). It is clear, however, that to measure the forces exerted by Zapruder on the camera, one may measure the change in the blurring of highlights in various frames.

Dr. Alvarez thought of this analysis in November 1966, and made some measurements on the frames in Life and Volume 18. He brought this matter to the attention of people he knew at CBS, and suggested that they have the work confirmed by an independent scientist. (CBS selected Dr. Charles Wyckoff of E.G. & G.) Even at that early stage of the analysis, Alvarez noted an oscillation starting around frame 180. CBS relied heavily on this as evidence for an early shot. Without getting into a detailed critique of CBS's work, I would accept this as probative evidence of such a shot. I feel that there is a great deal of evidence which suggests an early shot, such as the reaction of Mrs. Kennedy and other witnesses in the Zapruder film. Some critics suggest that the early shot hit Kennedy. As evidence for an early shot, the detailed measurement of the acceleration by CBS and Alvarez does not allow one to make a stronger statement than does the simple observation that there is suddenly a lot of blurring in the film. This observation was made in one of the first books on the assassination. Harold Weisberg noted (Whitewash, pages 47-48) that the film becomes suddenly fuzzy at frame 190, and attributed this to motion of the camera resulting from Zapruder's emotional reaction to what he had seen. Weisberg also pointed out that Zapruder testified (7H751) that he heard the first shot and saw the President lean over and grab himself, which suggests that the shot came well before the President disappeared from Zapruder's view about frame 210.

(Incidentally, this book had been made available to CBS while they were working on their report. Thompson notes that an analysis along the same lines as Alvarez' was considered and rejected as probably inconclusive by Life in November 1966. However, Alvarez' work was completely independent. Until it was televised I did not even know that he was doing such a study.)

The main argument about this "jiggle theory" is not about the probative evidence for an early shot, but about the claim that it can prove that there were three and only three shots. If the film is a sensitive enough indirect record of shock waves, it could provide an accurate count of the number of shots. Before coming to such a conclusion, as CBS did, I think it is necessary to consider all of the following questions:

(A) Is the oscillation seen on the film characteristic of the reaction to a shot?

(B) To what extent can other effects produce similar oscillations?

(1) What is the "noise level" - that is, if an undisturbed person holds a camera "steady", how much acceleration is measured by the technique of examining blurs? How often, if at all, does the oscillatory pattern associated with a shot occur if there is no disturbance?

(2) Do imperfections in the camera mechanism produce ~~xxxxxxx~~ blurring? (Thompson said (page 293) that the experts at Life felt that some blurs were so caused.)

(3) Does a conscious reaction (e.g., a small change in the panning) or a small disturbance (crowd noises, etc.) produce a reaction like that of a shot?

(C) Under what conditions will a shot not be recorded by this method?

(1) Will a shot with a weak sound at Zapruder's location produce a recognizable signal? (This is important if one wants to rule out, say, a shot

from the TSBD that went wild.) Would a shot from a weapon other than an ordinary rifle, or one with a "silencer", muffle a shot enough to produce no measureable reaction?

(2) Perhaps the most important question: what is the "resolution time" of this method of detecting shots? That is, what is the minimum separation time for two closely-spaced shots that will still be recorded as distinct? There are two different aspects to this problem:

(a) Does the neuromuscular reaction which triggers the oscillations have a non-negligible "dead time"? Perhaps only the first of a series of closely spaced shots will cause jiggling, with subsequent ones not triggering a new reaction.

(b) If a new oscillation is started while a previous one is dying away, can it be detected with measurements of whatever precision is available, or will it just look like an extension of the previous oscillations, with no discernible starting point?

There is no doubt that each of these two imperfections in the method does make some contribution to the resolution time. (I have not tried to calculate what these times might be.) ~~xxx~~ This is not just a minor quibble, since the observed oscillations which have been attributed to shots extend over a large time - on the order of a second. If a pair of shots a second apart would be missed by this method, there is no way of claiming that there were only a certain number of shots.

Alvarez and CBS did consider point (A) adequately, I think. Tests were made by firing shots near cameramen who were told to hold steady as they were filming. Although this test did not reproduce the conditions of the assassination, it was close enough. Also, Alvarez noted the pattern of blurring after frame 313, where there certainly was a shot, and saw a general similarity to the two other patterns which he attributed to shots. This justifies the conclusion that the film strongly suggests an early shot.

As far as I know, CBS did not do any separate tests on point (3). They did examine the very early frames of the film, from which a "noise level" can be estimated. However, I feel that the arguments which CBS used to distinguish the three alleged shots from other oscillations do not stand up.

On the broadcast CBS report, Wyckoff almost said that the characteristic blurring pattern occurs only three times:

> RATHER: ... Now, how many times does that occur in this ten-second stretch of 8 mm film?
> WYCKOFF: Now, that - that occurs several times, as we've represented up here on the - on this film on the wall. It occurs at frame 190, it occurs again at frame 227. Frame 227 was the next evidence that I had of Mr. Zapruder moving his camera. And it occurs at frame 313.... (White, p. 228)

White's text (pp. 75-6) makes clear that there were other candidates for shots:

> Mr. Wyckoff, meanwhile, reported that he had found similar streaking [to that on the test film] at three points of the Zapruder film [frames 190, 227, and 313] and other streaking, far less marked and easily distinguishable from the principal streaking, in two other places on the film.

I have never seen Wyckoff's actual measurements, and am unwilling to simply accept his judgment, as reported by White, as to which streaking was "principal." The problem CBS set for itself was not to identify shots, knowing that there were three, but to try to determine how many there were. White provides some further analysis of the two "non-shots"-

> The two minor disturbances also included one point of interest. They were represented on a chart drawn by Mr. Wyckoff as he proceeded with his tests. It may be seen that the disturbances at two particular frames are significantly less noticeable, in terms of the length and duration of the streaking. [I don't have this chart.] One frame provides nothing further than its own mute evidence. But the other frame shows a slight, almost imperceptible slowing down of the President's car, followed almost immediately by resumption of its normal parade speed. Professor Alvarez hypothesizes that both the deceleration and the twitch of Mr. Zapruder's hand were brought about by a siren, which caused the driver instinctively to lift his foot from the accelerator ... and at the same time slightly startled Mr. Zapruder. A tenuous hypothesis, to be sure, but not unreasonable. The earlier slight disturbance in the first frame was in all possibility (!) caused by still another unexpected sound, which cannot be identified. [A gunshot, maybe?] In that fashion, all the evidence recorded on the Zapruder film is accountable. (page 76-77)

Alvarez measured the car's speed, and did find that it slows down somewhat near frame 290, where one of the weak oscillations starts. I independently confirmed that the car slowed down, but I could not pin the deceleration down to a single frame or so, and found no evidence of a subsequent rapid return to

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a greater speed. However, Wyckoff told me

> I might mention at this point that I could not substantiate Dr. Alvarez' finding that the President's car slowed down measurably some time prior to the fatal shot. I did find irregularities in the velocity but nothing suggesting a definite trend in change of speed.

The siren hypothesis is tenuous, to say the least. I know of no evidence that the siren started about a second before the fatal shot, as CBS hypothesized, and Thompson has pointed out considerable evidence that one went off only after that shot. Thompson plausibly attributes the deceleration to the fact that the driver looked back over his shoulder at that time.

At most, the evidence is "accountable" if one wants to prove consistency with three shots. As far as I know, CBS did no control experiments to study the effect of small disturbances, and the resolution time problem was not considered at all. In my opinion, this totally invalidates the impression they wanted to give of having done a good scientific test which proved three shots. Their conclusion was presented with the backstopping of Alvarez' opinions:

> STOUT: What does this finding mean to those of us who simply have followed the controversy over the assassination, and are not physicists? [Or have been totally confused by CBS' explanation of the physics - P.H.]
> ALVAREZ: Well, to me, it means that there were indeed three shots fired, as the Commission said; that the one that apparently didn't hit anyone in the car was fired before the one that hit the President, and not between the two shots that obviously hit the President. (page 228, emphasis added)

Alvarez' opinion was clearly based not only on the Zapruder film but on the other evidence presented by the Commission. As such, his opinion is of no special value because he is a physicist, and should be set against the contrary opinions of those who (unlike Alvarez) have studied the other evidence in detail. Perhaps he assumed that CBS would do a more thorough study of the possibility of more than three shots than they actually did. For CBS to rely on his opinion as authority for the "scientific" conclusions is neither good science nor good journalism.